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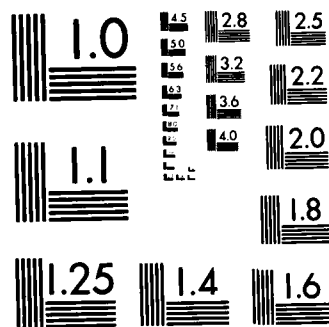
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## VETERAN STATUS AS A SCREENING DEVICE: COMMENT

In a recent article in this review, Dennis De Tray addressed the earnings premium of veterans, a premium sometimes observed empirically.<sup>1</sup> The causes of the premium as well as its magnitude are policy questions of growing importance because our military is manned with young people -- currently over 85 percent are under 35 years of age. In the next decade, as the size of the youth cohort shrinks both proportionally and in absolute magnitude, the wages of the labor pool on which the military draws are expected to rise. While changes in relative wages across age cohorts induce changes in the age mix of employees, the military is generally assumed to have smaller substitution possibilities than other sectors in the economy. Thus the military is expected to employ an increasing fraction of the young male population.

The magnitude of deferred returns from military service is important for evaluating the viability of the All-Volunteer Force. Attractiveness of military enlistment depends upon the value of military compensation relative to civilian compensation, transferability of military training, and the value of veteran status as a signal to potential civilian employers.

De Tray's test of the value of the veteran status signal is both conceptually and empirically flawed. In this note, we examine De Tray's tests. First, we discuss the logical inconsistency of De Tray's central

hypothesis that the proportion of veterans in the civilian population should be positively related to the veteran premium. Second, empirical problems, including measurement error, incorrect specification, and test interpretation are discussed. We supplement De Tray's tests by considering the value of veteran status as a certification screen. Finally, we construct our own test of veteran status as a signal; our results lead us to reject De Tray's conclusion that veteran earning premiums are importantly due to the signalling value of veteran status.

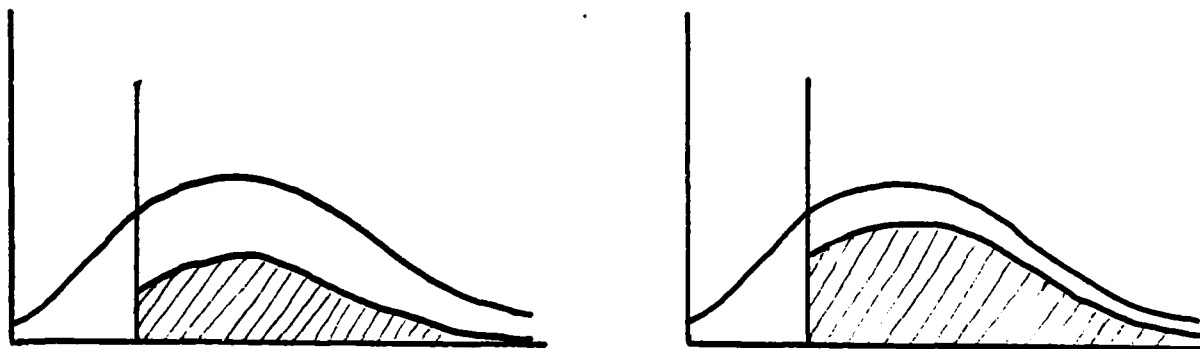
#### I. PROPORTION VETERAN AND THE VETERAN PREMIUM

De Tray argues that veteran status could be a meaningful signal to employers for two principal reasons: veterans' mental and physical qualifications are greater than average for their cohort (minimum standards for military service screen out the bottom of the distribution), and veteran status conveys the ability to successfully complete a job (the certification effect). From these observations, he formulates three hypotheses; the primary one states that the "effect of veteran status on civilian earnings will be a positive function of the proportion of men in a given population who claim veteran status" (De Tray, p. 135). It is important to note that this hypothesis does not necessarily follow from the existence of a certification effect. The signal conveyed by successful completion of military service should be independent of cohort proportions.

The signalling function that De Tray tests for his primary, as well as his secondary hypotheses, follows from the mental and physical standards required by the military. Figure 1 illustrates how information from the mental and physical screen is improved when a larger proportion of the cohort has veteran status. The larger the proportion of the cohort who have passed the screen, the higher is the probability that those without military service could not pass the military screen.

Figure 1

Veteran Proportion and Quality Distribution  
of Non-Veterans\*



\*Veteran proportion in cohort is shaded.  $Q^*$  is the minimum military standard.



Our conceptual problem concerns the paucity of information conveyed to employers by this screen. The screen indicates that veterans meet at least minimum physical and mental standards (or they did upon entry to the service). The employer can only estimate the probability that non-veterans did not meet minimal standards. Moreover, to estimate this probability, the employer needs to know the proportion of the cohort who are veterans. In contrast, direct employer testing would provide precise quality measures which employers could compare directly to relevant standards for each job.

How expensive, then, is direct employer testing? Is it reasonable that measurable pay differentials would arise because of information from military mental and physical screens? The tests administered by the military are not lengthy: the physical examination takes about one hour and the mental examination three hours. If standardized tests provide information, it would seem cost-effective for employers to test potential employees themselves. Or if, as De Tray implicitly suggests, employers collectively find such a single screen useful, it seems that economies of scale would dictate that a national organization, like the Educational Testing Service, would respond to private demand and establish testing facilities as they have for college entrance. In short, we find it implausible that employers are willing to pay permanent wage differentials for the imperfect minimum-standards signal of veteran status.

## II. DE TRAY'S EMPIRICAL ESTIMATES

De Tray does, however, obtain some empirical support for his hypothesis. Before discussing the measurement errors that we suggest generate his results, it is worthwhile to discuss his procedure. Using samples from 1960 and 1970 Census data for black and white males, De Tray estimates four sets of earnings regressions.<sup>2</sup> For black males in 1960, for example, he estimates a separate regression for each of 11 four-year birth cohorts, controlling for schooling, age, veteran status, southern and central city residence.

De Tray then takes the 11 separate coefficients for veteran status and regresses them upon the proportion of veterans in each cohort sample.<sup>3</sup> Since his hypothesis is that the veteran premium is higher the larger the proportion veterans in the cohort, he hypothesizes, and usually finds, a positive sign on the veteran proportion variable.

### A. MEASUREMENT PROBLEMS

The first problem is that De Tray's measure -- proportion of a working sample claiming veteran status in each age group -- is a poor proxy for the proportion of each cohort successfully passing the military's mental and physical screen (the shaded portion of the densities in the above diagrams). De Tray's sample excludes the self-

employed, the unemployed, and those out of the civilian labor force. For young cohorts, De Tray's veteran variable systematically understates the proportion who successfully passed the screen: many individuals are still in the military or in school (utilizing the G.I. Bill) and thus outside of the sample.

For older cohorts, the constructed variable has equally serious measurement problems. The military is a youthful organization, especially with respect to entrants. Even in World War II the military accessed virtually no new entrants over the age of thirty. Thus, the results of the screen are defined for any cohort at about thirty years of age -- when the military stops testing and certifying individuals. One might suspect, then, that the measurement bias eventually goes to zero (as individuals leave the military and show up in De Tray's proportion). But, De Tray's constructed variable fails to stabilize; it continues to grow as the cohort ages.

Particularly for blacks, the constructed variable produces some strange results. For example, in 1960, slightly over one percent of black males aged 42-43 were in the military.<sup>4</sup> If they all left in the period between 1960 and 1970, De Tray's method should identify only a one percent increase in the proportion veteran. However, in De Tray's sample, the proportion veteran jumps from 43 percent in 1960 to 50 percent in 1970 (black males aged 42-43 in 1960 and 52-53 in 1970). Either individuals are incorrectly remembering they were veterans, or else the data have serious selectivity problems.

Measurement errors in independent variables generate inconsistent parameter estimates. Moreover, De Tray's measurement errors are not random. Rather they systematically understate the true proportion of the population with military service for the younger age groups -- groups for which De Tray has small values of the dependent variable. For the older age groups, measurement errors in veteran status are often positive, and the older age groups obtain larger premiums for veteran status. Thus, the dependent variable is positively correlated with the measurement error in the independent variable, creating a spurious upward bias in the regression coefficient.

#### B. MISSPECIFICATION

In addition to measurement problems, there is a potentially serious misspecification in the wage equations. De Tray does not control for government employment. Controlling for government employment and interacting this variable with veteran status are appropriate since the federal government explicitly gives preferential treatment to veterans. As table 1 shows, veterans are substantially more likely than non-veterans to be employed in the federal sector. Moreover, there is evidence that federal employees receive a wage premium relative to comparably qualified private sector workers (see, for example, Sharon Smith). As a result, the veteran status variable may be reflecting this federal wage premium. We will return to this point in a later section.

TABLE 1  
EMPLOYMENT SECTOR DISTRIBUTION

<u>% Employed</u>	<u>Federal</u>	<u>State</u>	<u>Local</u>	<u>Private</u>
White				
Veteran	6.7	3.7	9.1	80.5
Non-veteran	2.3	4.3	9.0	84.4
Black				
Veteran	12.5	4.9	12.8	69.8
Non-veteran	2.7	2.7	11.5	83.1

Source: March 1976, Current Population Survey. All black and white males aged 22-65.

#### C. INTERPRETATION PROBLEMS

As a test of screening versus human capital explanations of veteran earnings differentials, De Tray proposes an examination of changes over time in a cohort's veteran premium. De Tray suggests that, if the veteran premium is due to differential human capital investment behavior, the veteran premium will increase over time as returns on earlier investments are recouped. De Tray also suggests that the screening hypothesis predicts change over time in a cohort's veteran premium only if there is a significant change in the proportion veteran. We have already discussed conceptual and measurement problems with the variable "proportion veteran" for a screening test. Here, we will quarrel with De Tray's interpretation of the predictions of the human capital and screening models.

De Tray finds large positive changes in the 1960 to 1970 veteran premium for white cohorts aged 22-27 (in 1960) and positive changes for black cohorts age 22-29 (in 1960). Since his veteran-proportion-measure exhibits the largest changes for these younger age groups, De Tray concludes the screening interpretation has some empirical support. Additionally, he concludes that a human capital model is not appropriate since it would predict longer periods of earnings growth. Our conclusions are different.

One could easily interpret the results of the experiment as supporting the human capital hypothesis. As previously discussed, the true cohort veteran proportion is essentially constant over time. Only the improper veteran proportion that De Tray measures is changing as people leave the military and enter the civilian labor force. Thus, the proportion veteran cannot be part of a test to determine why cohort earnings change.

Earnings changes for older individuals are generally small and hard to estimate. As a result, we are not surprised to find statistically insignificant changes in the veteran premium for older cohorts. Only when earnings are growing rapidly (when individuals are passing from their twenties to their thirties) are the data robust enough to identify the source of differences in earnings. In short, we would emphasize the large positive changes in the veteran premium for the younger cohorts.

rather than the small effects for older cohorts, and argue that this is quite consistent with a human capital interpretation.

While we object to De Tray's interpretation of the 1960-1970 veteran premium comparisons, the results do not unambiguously favor the human capital hypothesis. De Tray has ignored the possible screening effect of sorting individuals into jobs with steeper earnings profiles. (See Riley for an elaboration of this point.) For example, veteran status as a certification screen could induce employers to prefer veterans for jobs for which there is a high company investment in on-the-job training.

Finally, because De Tray tested all of his other hypotheses by regressing the estimated veteran coefficient against the proportion veteran, we find the evidence for these hypotheses as unconvincing as that for the primary hypothesis. As a minor aside, however, we have difficulty accepting the assumption of one hypothesis. De Tray states that because schooling quality for blacks is more varied than for whites, veteran status should be a more useful screen for blacks. His assumption concerns within-cohort variation in schooling quality, not the more commonly identified variance in schooling quality over black cohorts of different schooling vintages. Cross-section schooling quality proxies we identified suggest his assumption is probably not correct. Schooling expenditures per pupil, historically used as a proxy for school quality, and Armed Forces Qualifying Test results, which

index basic verbal and numeric skills, exhibit a lower variance as well as a lower mean for predominantly black schools. (See Link and Ratledge and OASD, Profile of American Youth.)

### III. ALTERNATIVE TESTS OF VETERAN STATUS AS A SCREENING DEVICE

In a world of perfect information, signals for labor quality would be redundant. In a world of imperfect information, however, signals can convey otherwise unattainable information about a potential employee's labor quality. Most signalling models, in fact, rely upon the asymmetry in information between the potential employee and the potential employer: although the employee knows his labor quality, the employer does not. Here we perform several tests of veteran screens. These tests do not definitively solve the partitioning problem, but results suggest that the signalling value of veteran status is not as important as human capital and institutional factors in explaining veteran earnings and employment choices.

#### A. Certification Screen and Wage Premiums

As one test of the screening effects of veteran status, we estimated logarithmic wage equations for 22-65 year old black and white males. The data used were from the 1976 March Current Population Survey. The individuals, like De Tray's, are white or black employed males, aged 22-65. All observations are weighted appropriately. Table



2 reports the full regression results.<sup>5</sup> For blacks, the veteran status coefficient is small and statistically insignificant, as are the interaction terms involving veteran status. This is in direct contrast to De Tray's findings and his second hypothesis, and confirms our earlier concerns about the misspecification of De Tray's wage equations.<sup>6</sup> Apparently, at least for the black sample, much of the veteran premium that De Tray estimates is, in fact, a federal government wage premium.

TABLE 2  
ln WAGE REGRESSION<sup>a</sup>

	<u>White</u>	<u>Black</u>
Constant	-.001 (6.8)	-.006 (0.1)
Age	.078 (57.0)	.057 (6.3)
Age <sup>2</sup>	-.0009 (44.5)	-.0006 (7.0)
Urban	.124 (17.0)	.232 (7.4)
South	-.068 (9.3)	-.205 (7.7)
School	.027 (9.1)	.020 (1.3)
Age.School	.0005 (8.4)	.00009 (0.3)
Hours	-.010 (27.7)	-.003 (2.1)
Local	-.107 (6.4)	.003 (0.1)

TABLE 2 (Cont'd)

	<u>White</u>	<u>Black</u>
State	-.146 (6.3)	-.177 (1.8)
Fed	.123 (3.8)	.274 (2.9)
Vet	.062 (4.5)	.024 (0.6)
Vet.Local	-.017 (0.8)	.044 (0.6)
Vet.State	.027 (0.8)	.060 (0.5)
Vet.Fed	-.006 (0.2)	-.086 (0.8)
Grad	.050 (3.5)	.082 (1.8)
Vet.Grad	-.025 (1.6)	-.007 (0.1)
R <sup>2</sup>	.33	.32
N	19,773	1,654

<sup>a</sup> t statistics are in parentheses. The variables Age, Age<sup>2</sup>, School (years completed) and Hours (of work per week) are conventionally measured. Urban, South, Local, State, Fed, Vet, and Grad (high school graduate) are dummy variables which assume the value one if the individual is in the appropriate category. Similarly, the dummy interaction variables (Vet.Local, Vet.State, Vet.Fed, Vet.Grad) assume the value of one for individuals in the category. Finally, Age.School is Age multiplied by School.

The regression results for the white sample indicate that there is a significant veteran premium. Table 3 reports the partial effect of

veteran status on the natural logarithm of wages for private sector and federal government employees. The base group in this table is composed of non-veteran, non-graduate private sector employees. As can be seen, the effect on wages of federal employment is substantially larger than that of veteran status or high school graduation. Nonetheless, the veteran status premium remains, even when interactions are considered.

TABLE 3  
PARTIAL EFFECTS ON  $\ln$  WAGES FOR WHITE MALES

	HS Graduate		Non-Graduate	
	<u>Veteran</u> (1)	<u>Non-Veteran</u> (2)	<u>Veteran</u> (3)	<u>Non-Veteran</u> (4)
Private	.087 (6.2)	.050 (3.5)	.062 (4.5)	--
Federal	.204 (9.3)	.173 (5.0)	.179 (8.1)	.123 (3.8)

Source: Regression coefficients in table 2. t statistics are in parentheses.

Earlier, we dismissed the ability signal, based on the proportion veteran, because of the paucity of information it afforded employers compared to tests which could be administered at nominal cost. We would not dismiss the certification effect of veteran status so lightly. Here we refer to the completion of military service. Because it is more difficult for employers to obtain information on quitters and stayers, we might expect employers to use this screen to select potential

employees. The information gained might be similar to the signal of high school graduation.

If the learning process in school is continuous, then a high school graduation dummy variable in an earnings regression which controlled for years of school identifies a "reliability" signal. Table 3 shows evidence of the "graduation" effect. Moreover, if the interaction between veteran status and high school graduation is negative (i.e., the signals are substitutes), it provides some support for the veteran status signal argument. The value of this screening test depends, of course, upon the extent to which veteran status and high school graduation convey similar information to employers. For the white sample, we do find a negative interaction between graduation and veteran status which verges on statistical significance, suggesting weak support for veteran status as an alternative screen. The magnitude of this effect, however, is quite small.

#### B. Employment Sector Selection

Our final test of the signalling value of veteran status follows from the asymmetry of information between employers and employees. We assume an employee knows his own labor quality; potential employers, however, do not. If veteran status is a screen, i.e., employers gain useful information about labor quality from veteran status, then a wage premium for veterans will exist (over and above any

premium due to human capital formation as a result of military training). In contrast, there is no informational value to an individual who chooses to be self-employed. Since a screen improves employer information only, we hypothesize that veterans should be less likely than non-veterans to be self-employed if veteran status is a valuable screening device.<sup>7</sup>

TABLE 4  
THE EFFECT OF VETERAN STATUS ON EMPLOYMENT OUTCOMES<sup>a</sup>

	<u>Private</u>	<u>Federal, State or Local Government</u>	<u>Self-Employed</u>	<u>Unemployed or Out of Labor Force</u>
Whites				
Partial derivative	.0172	.0378	-.0019	-.0530
(t-statistic)	(1.20)	(3.55)	(-.21)	(-6.15)
Distribution	69.9%	12.9%	9.1%	8.1%
Blacks				
Partial derivative	-.0405	.0720	-.0022	-.0294
(t-statistic)	(-1.73)	(4.06)	(0.28)	(-1.56)
Distribution	64.3%	15.3%	2.5%	17.9%

<sup>a</sup> The model was estimated controlling for school, age, urban and south. Complete results are available for authors.

We test this hypothesis using the same 1976 Current Population Survey data discussed earlier. Table 4 presents the partial effect of veteran status on employment sector calculated from results of a multinomial logit model of sector choice. The results suggest that the choice of the self-employment sector is independent of veteran status.

Since veterans are as likely as non-veterans to be self-employed, we can reject the hypothesis that the sole value of veteran status is informational. Further support for the value of veterans' human capital comes from the results for the fourth sector choice -- namely, those not employed. Veterans are significantly less likely to be in this category.

Not surprisingly, the results indicate that veterans are significantly more likely than non-veterans to gravitate to the government sector: other things equal, white veterans are 4 percent more likely, and black veterans 7 percent more likely, than non-veterans to be in the government sector. As previously noted, the government sector, by law, screens on veteran status. This screen, however, is not informational; it is an institutionalized reward for service to country.

#### IV. SUMMARY

It is very difficult to empirically disentangle productivity-related earnings differentials from the "signalling" earnings differentials suggested by Spence. Indeed, signalling can be viewed as an attempt to capture human capital characteristics which are not readily measured or observed.

We do not claim to have definitively explained veteran wage premiums. However, our review of De Tray's hypotheses suggests that his

are poor tests. We have looked at the certification aspect of veteran status signalling, the government wage premium and veteran preference, and the implications of veteran screening for employment sector choice. Our results lead us to reject De Tray's conclusion that the "overall picture... is one of veteran status as a valuable screening device" (De Tray:141). Instead, it appears more likely that military service imparts human capital. In summary, the jury is still out, but productivity enhancement resulting from military training is a likely verdict in the veteran premium case.

#### FOOTNOTES

\*Dr. Aline Quester, Ms. Jean Fletcher, and Mr. Alan Marcus, Center for Naval Analyses.

<sup>1</sup>An earlier review of empirical work concluded that the effect of military service on civilian earnings is negative or insignificant (see Kassing). Little and Fredland found some evidence for veteran premiums.

<sup>2</sup>De Tray uses a data set drawn from the 1/100 Public Use Census tapes. The data was created by Smith and Welch.

<sup>3</sup>In most of his regressions, he appropriately weights the variables by the inverse of the standard error of the regression coefficient. All regressions, however, should be so weighted; to weight only when the procedure "significantly improved the efficiency of the equation" is not acceptable (De Tray, p. 135).

<sup>4</sup>The population in the military is calculated as the difference between the "Total Population including Armed Forces" and the "Civilian Population" (U.S. Bureau of Census).

<sup>5</sup>The appropriate methodology for estimation of these wage regressions would control for selection into the various employment sectors. Estimates using selection-correction techniques were plagued with identification problems and are not reported here.



<sup>6</sup>An F test of the joint value of the veteran variable and all interactions showed joint insignificance. Actually, in less than one third of De Tray's wage regressions for blacks (22 regressions for different age/year groups) is the veteran coefficient statistically significant. De Tray's conclusion that veteran status is a more important screen for blacks than for whites comes from his exercise regressing the generally insignificant veteran premium coefficient against the variable "proportion veteran".

7 An alternate test would be an analysis of veterans' wage premiums in the self-employed sector, but data on wages for this sector are too imprecise to be useful.

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